allm

611 West 113 th St
New York 25, N.Y.

September 18, 1949

Dear Josh:

I know I ought to have written to you long ago. You and your wife did a lot more for me than most people do for a house-guest, and I enjoyed the visit more than I ever enjoyed a visit as a house guest. And the instruction and ideas you gave me have been a tremendous help since I have returned.

Meningococci grow on the synthetic medium, and as you probably noticed in the recent JB, they don't even require glucose. But 48 hour growth on solid simple medium is not so heavy as 10 hour growth after the addition of 1% serum. The first lot of medium I made up didn't show so much difference, or I didn't notice it, but Now I'm not satisfied with the synthetic and I am trying to find out what the difference is. Both crystalline albumin and thiamin improve growth slightly, but not to compare with the serum.

At first I went ahead on developing penicillin-resistance, and increased it in two strains to four-fold, but then the thermostat went bad and the incubator got too hot one night and killed off all my strains. Since then I have been working with the medium, and trying to develop a good differential for fermentation. Most dyes inhibit growth in very low concentrations, but I have just started to see if I can develop a strain that is tolerant to EMB. That may be useless, because they produce acid only for a short time, and then become alkaline. I wonder if you could send me a very small sample of tetrazolium, and tellme where you now get your supply.

In Miller's experiments on developing penicillin resistance, he found that he could step it up faster by taking growth from the second-highest concentration on which they grew at each step. He assumed this was because of the size of the inoculum, but didn't report any attempt at measuring the inoculum. That could be the explanation, but another possible explanation is that where many mutants grew, and were transferred, they had a chance to recombine and get the benefit of several additive mutations for the next stage. For the present, that is the thing I have my sights set on rather than virulence.

While I was there you asked me for suggestions on how to analyze your unexpected distribution phenomena. I haven't been able to hit on any approach you are not already using. I would expect the best results to come from the new HFR strain where there is no question of uniform bias, although I know you have been using reciprocal crosses. And you say you have obtained other heterozygote strains; of course they have biochemical deficiencies, but I shall want to hear more about how they behave.

I'll write to you about any really interesting results I obtain whenever I obtain them. Thanks again for all your help and thoughtfulness.

Sincerely yours,

John Allen